Research Methods in Applied Behavior Analysis

Issues and Advances

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Simulation Research in the Analysis of Behavior

ROBERT EPSTEIN

INTRODUCTION

The more interesting some instance of human behavior, the more difficult it is to analyze (perhaps that's why we call it interesting). And where objective analysis is difficult, fictions turn up. Consider the following cases: at age one, most children react to their mirror images as if they are seeing other children; by age two, most children react as if they are seeing themselves. How can we account for the change? Does it help to say that the child has developed a "self-concept"?

Virtually all human beings acquire language and, by age five, have rich vocabularies. They also seem capable of emitting an infinite number

This chapter is dedicated to the memory of Don F. Hake who had agreed to write the chapter on simulations for this volume. His death in August 1982 made this impossible. Dr. Hake was a pioneer in the study of cooperative behavior with human subjects and would surely have discussed some of his innovative work in this area. Rather than try to anticipate what he might have said, I have concentrated more on my own work. I urge the interested reader to consult some of Professor Hake's writings directly (e.g., Hake, 1982; Hake & Olvera, 1978; Hake & Vukelich, 1972, 1973). Portions of this chapter were included in an invited address entitled "The Self-Concept and Other Daemons," which was given at the 8th annual meeting of the Association for Behavior Analysis, Milwaukee, May, 1982 (Epstein, 1982b, Epstein & Koerner, 1986).

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of different sentences. How can we explain this? Does it help to say that we are born with "language organs" or that a set of "cognitive rules" is guiding us?

A 2-year-old girl is faced with the proverbial "marble-under-the-couch" problem: She stretches toward the marble but cannot reach it. After repeated attempts, she looks around the room and reaches suddenly for a nearby magazine. She casts about with it until she knocks the marble out from under the couch. Do we shed light on this behavior by attributing it to "insight" or "reasoning"? If not, what contribution, if any, can we make?

An audience of cognitive psychologists has listened with adoration to a prominent colleague. A member of the audience, known for his wit, raises his hand, stands, and deadpans, "But how is this relevant to *pigeons*?" There is a swell of laughter and some applause. Could we predict who would laugh? Does it help to say that someone has a "sense of humor"? (Did *you* laugh?)

These and many other instances of complex behavior in people are difficult to analyze for several reasons. First, they are all multiply determined at the time they occur. Sofa, marble, magazine, toys, television, and so on, strengthen many behaviors, and the child's own behavior changes the environment and hence changes the probability of subsequent behavior. Second, they are the result of complex environmental histories and, presumably, biological factors. Language is acquired haphazardly over a period of years, and, though speaking and speaking grammatically may not be systematically taught, it is more effective than not speaking or speaking ungrammatically; in other words, children are exposed from birth to subtle and complex "contingencies of reinforcement" that support speaking and speaking grammatically. Modeling, instructions, and physical maturation also undoubtedly make important contributions. Third, they are all typically human phenomena; problem solving, language, wit, the behaviors that come under the rubric of *self-awareness*, and so on, are all relatively rare in nature; the study of nonhuman organisms is not as informative as it is for simpler behavioral phenomena. And finally, because the histories are complicated and the phenomena relatively unique to humans, it is difficult, if not impossible, to explore them through experimentation.

Similar problems are faced in many domains of scientific inquiry. Complexity (say, in meteorology), the importance of events in the remote past (say, in evolutionary biology), inaccessibility (say, in astronomy), or ethical considerations (say, in neurology) often prevent direct study. Fortunately, methods have evolved which allow at least some tentative analyses. This chapter concerns one of the most powerful of such

methods—the simulation—and its application in the analysis of complex human behavior.

ONE HUNDRED BABIES

B. F. Skinner once told me that an Indian (Asian) tried to induce him to move to India by offering him 100 babies with which to do research. As appalling as the offer may sound, without those babies some of the most interesting questions in the analysis of behavior can never be anwered definitively.

Let us say, for example, that you are interested in the origins of language. If you took an extreme nativist position, you might assert that spoken language would emerge even if a child were never exposed to it—as, presumably, would walking. How would you test such an assertion? You might wait for a naked child to appear at the edge of the woods, but you would have a long wait and could never be certain of the child's history. The handful of feral children that have turned up have not shed light on the issue; the so-called "wild boy of Burundi," for example, was indeed mute but turned out to be brain damaged, autistic, and profoundly retarded (Lane & Pillard, 1978).

More definitive answers could come only from carefully conducted deprivation studies. One would have to raise some children from birth without exposing them to language (taking care, somehow, to deprive them of nothing else). A positive result would be extremely informative: If the children came to make sounds that had characteristics of known languages, your hypothesis will have been supported. Perhaps nonlinguistic sounds of certain frequencies were responsible; we could control for that possibility with still other children. A negative result would be less informative: Perhaps we inadvertently deprived the children of something besides the sound of language.

According to Salimbene, a medieval historian, the Roman emperor Frederick II conducted such an experiment in the 13th century:

> His . . . folly was that he wanted to find out what kind of speech and what manner of speech children would have when they grew up, if they spoke to no one beforehand. So he bade foster mothers and nurses to suckle the children, to bathe and wash them, but in no way to prattle with them or to speak to them, for he wanted to learn whether they would speak the Hebrew language, which was the oldest, or Greek, or Latin, or Arabic, or perhaps the language of their parents. . . . But he laboured in vain, because the children all died. For they could not live without the petting and the joyful faces

and loving words of their foster mothers. (quoted in Ross & McLaughlin, 1949, p. 366)

We are better off, some people say, not knowing the answers to certain questions. This issue aside, we can know the answers to certain questions in the analysis of behavior only by employing extreme and entirely unacceptable methods of the sort Frederick (the one with the "K") was said to employ. For all practical purposes, then, we can never develop definitive accounts of certain complex human behaviors (though it is a useful exercise to devise the necessary methods).

This sad pronouncement applies to all of the examples of complex behavior I gave above, as well as to countless others. You may suspect, for example, that a child can not efficiently solve the marble-under-the-couch problem unless the child has already learned—perhaps through shaping, modeling, instructions, or some combination of these—both to grasp objects and to make contact with objects using other objects. Again, how would you test such a hypothesis? Simply testing a child who lacks such skills before and after you have established those skills would not be adequate, for you would still somehow have to control for prior learning.

It is a truism that all scientific pronouncements are tentative. But some are far more tentative than others. If we could carefully control and monitor all of the conditions that we believed to be relevant to the emergence of some behavior—genes, learning experiences, nutrition, and so on—we could establish with greater confidence the contributions of each. In cases in which we cannot, for some reason, experiment directly, we must resort to indirect methods. Which brings us to the laboratory simulation.

SIMULATIONS IN THE SCIENCES

As is the case in the analysis of behavior, the most interesting questions in the natural sciences are the most difficult to analyze. The origin of the universe, of life, and of species is still attributed by many to a deity, and not only is it impossible to disprove such a theory, it is equally impossible to prove an alternative. Scientists bring diverse methods and information from many fields to bear on such questions. One helpful method is the simulation. Consider some examples: in the 1950s the biologists S. L. Miller and H. C. Urey tested a theory of the origin of life by simulating some of the conditions believed to be typical of

primitive earth. The "soup" they prepared contained no organic materials at first but soon yielded both amino and hydroxy acids, important precursors of life as we know it (Miller & Orgel, 1973). They did not *prove* that the theory was correct; they merely proved its *plausibility*. In recent years, new geological and other data have revised our conception of earth's primitive atmosphere. New theories of the origin of life are tested in laboratory simulations like Miller and Urey's (e.g., Pinto, Gladstone, & Yung, 1980). As is true in any domain of science, the dominant theory at any point in time will usually be the one that accounts for more data—in this case, a steady accumulation of data in several fields.

Recently evidence was presented that supports a rather fantastic explanation for the mass extinction of dinosaurs and other organisms that occurred on earth 65 million years ago. Some now believe that a large asteroid struck the earth and kicked up enough dust to darken the skies for several months, thus destroying vital food chains (Alvarez *et al.*, 1984). Critical evidence comes from laboratory simulations of large-body impacts (Kerr, 1981). Again, such simulations do not prove the theory, but, in conjunction with the fossil record and other geological data, they lend credence to it.

The computer has become one of the most powerful tools of simulation research. If the variables controlling some phenomenon are sufficiently understood so that it can be described in formal terms—so that laws in the form of equations or algorithms can be stated—the computer can be used to plot the course of extremely complex systems that involve many such phenomena. With accurate equations and parameters, the behavior of such systems can be predicted. Such is the basis of long-term prediction in meteorology, astronomy, and other sciences. In recent years, computers have been used successfully to predict the course of chemical reactions by utilizing laws of chemical and physical processes (Edelson, 1981). Computer simulations have also been used for many years in the social sciences—in economics, cognitive psychology, game theory, political science, and so on—but, as the introduction to a book on the subject points out, "the researcher must know a great deal about the real system before he can presume to simulate it" (Dawson, 1962, p. 14); where basic principles are still under investigation and formal statements are crude and simplistic, computer simulations are probably premature. It is true that you can, by accelerating processes or varying parameters, use computer simulations to discover things you did not know, but your results will be no better than the equations with which you started.

Most of the simulations I have mentioned have been attempts at aithful reproductions of certain phenomena in all their complexity—'causality-based description[s] combining the underlying fundamentals of the many components of . . . highly complex system[s]" (Edelson, 1981, p. 981). But as Edelson points out, the language of simulation and modeling is used in diverse ways. Some simulations mimic phenomena in relatively arbitrary ways. At one extreme are models that look or behave like something but whose resemblance is superficial and which have no predictive value. The circus animal that wears glasses and turns the pages of a book appears to be a reader but does not do these things for the same reasons a person does and is not affected by the words on each page as a person is.

The language of simulation is usually reserved for models that are at least predictive. Even predictive models, however, may have varying degrees of similarity to the object. An engineering text (Murphy, 1950) makes some useful distinctions, adapted somewhat for this discussion: A *true simulation* faithfully reproduces all significant characteristics of some phenomenon; Miller and Urey attempted a true simulation. An *adequate simulation* reproduces only some significant characteristics. A *dissimilar simulation* bears no apparent resemblance to the object but is still predictive. An electrical circuit, for example, can simulate characteristics of a vibrating mechanical system. Virtually all computer simulations fall in this category.

The computer simulation requires its own analysis, for though it bears no apparent resemblance to its object, it can represent formally any number of the object's characteristics. If it faithfully represents all significant characteristics—say, in the case of the marble problem, critical experiences, current stimuli, relevant principles of behavior, and so on—we might call it a *true computer simulation*. Edelson's (1981) simulations of chemical reactions fall in this category. If it behaves appropriately and is predictive but uses algorithms that may be unrelated to those that characterize the object—say, it produces various solutions to the marble problem simply by calling them up from memory—we might call it a *dissimilar computer simulation*, and so on.[1]

What follows is an example of what was intended as a true simulation of an instance of complex human behavior.

[1] I have heard such programs called, respectively, "simulation-mode" and "performance-mode." Weizenbaum's (1966) famous ELIZA program, which simulates a therapist, would be an example of the latter. Though it engages in fairly natural exchanges, no one would claim that it does so because it incorporates "true" models of language or therapy.

"SELF-AWARENESS" IN THE PIGEON

A variety of behavior is said to indicate that a person has a "self," "self-awareness," "self-knowledge," or a "self-concept." People tell you what they are thinking and where it hurts; at some point children recognize photographs of themselves and their reflections in a mirror; children will apparently imitate videotapes of themselves longer than videotapes of others; and so on (Gallup, 1968; Kagan, 1981; Lewis & Brooks-Gunn, 1979). Little progress has been made in accounting for such behavior. Kagan (1981) suggested that physical maturation is the key. Lewis and Brooks-Gunn (1979) and Gallup (e.g., 1979) attributed it to the development of a cognitive entity called the "self-concept."

Behavior with respect to one's mirror image is said to be a "compelling" example of the development of self. Such behavior is said to progress through a series of four stages, first noted by Dixon (1957). At first a child shows little or no reaction. When a few months old it begins to react as if it is seeing another child—by laughing, touching, and so on. The third stage, which Dixon (1957) called a period of "testing" or "discovery," is critical: Children often stare at their reflections while they make slow, repetitious movements of the mouth, hand, leg, and so on. Finally, by about age 2, most children react as if they are seeing themselves, at which point they are said to be "self-aware" (Amsterdam, 1972; Lewis & Brooks-Gunn, 1979). Amsterdam (1968, 1972) devised an objective test of such behavior: a child had to use a mirror to locate some rouge that had been smeared on its nose (which, presumably, it could not see directly). Chimpanzees, after extensive exposure to mirrors, also come to exhibit such behavior, though monkeys apparently do not, and it is claimed that only humans and the great apes are capable of it (cf. Epstein & Koerner, 1986). How can one account for the change?

This is another one of those origins problems. Without the 100 babies, one can use only indirect methods to determine the possible role of experience, physical maturation, and so on. The Miller and Urey approach could be used as follows: Suppose that success in the mirror test is due to some rather simple learning experiences, ones which chimps and children actually have before they are successful in the test (Gallup, 1970; Lewis & Brooks-Gunn, 1979). Perhaps they must acquire two behaviors—touching themselves where they must touch during the test, and locating objects in real space given only mirror images. One could test such a theory by establishing such behaviors in organisms that would normally be incapable of success in the mirror test and seeing whether they were then successful.

Epstein, Lanza, and Skinner (1981) did so with pigeons. Pigeons were taught over a period of a few days (a) to scan their bodies for blue stick-on dots and peck them and (b) to peck certain positions on the wall and floor of their chamber given only the brief flash of a blue dot in a mirror. A blue dot was then placed on each pigeon's breast and a bib placed around its neck in a way that made the dot invisible to the pigeon but visible to others when the bird stood fully erect (Figure 1). Each of three birds was observed for 3 minutes in the absence of a mirror and 3 minutes in its presence. Independent observers judged few or no "dot-directed" pecks during the first period and an average of 10 per

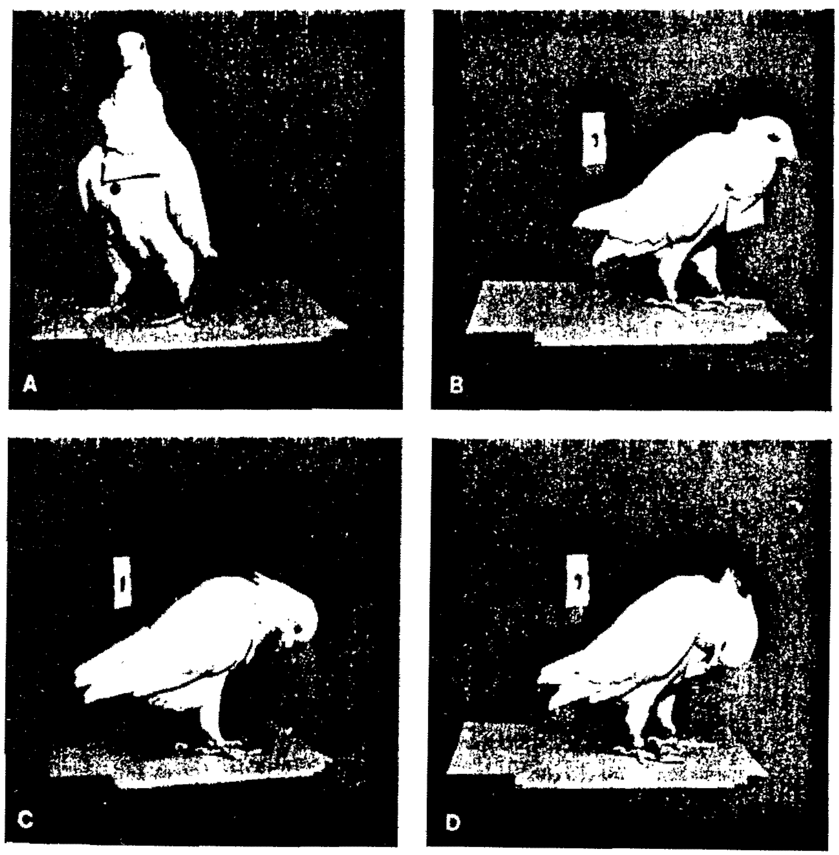


FIGURE 1. "Self-awareness" in a pigeon. (A) A dot is visible just below the bib with the bird standing fully upright. (B) The bird faces the mirror at right. The bib makes it impossible for the bird to see the dot directly. (C,D) The bird repeatedly moves toward and pecks the position on the bib which corresponds to the dot he has seen in the mirror.

bird in its presence. Even though no reinforcers were delivered during the test and though the birds had never before worn blue dots when exposed to the mirror, it seemed that each pigeon was now able to use a mirror to locate a spot on its body which it could not see directly. We thus proved the plausibility of our hypothesis, just as Miller and Urey had proved the plausibility of one theory of the origin of life.[2]

THE COLUMBAN SIMULATIONS

There are at least four classes of behaviors that have resisted analysis—covert behaviors (thoughts, feelings, images); complex, typically human behaviors that are difficult to trace to either environmental or biological factors (language, the behavior that comes under the rubric of "self," problem-solving behavior); behavior controlled by temporally remote stimuli (which leads some people to speak of "memory"); and novel behavior ("creativity," "productive thinking") (Epstein, 1985a). As I noted above, complexity, inaccessibility, the importance of events in the distant past, ethical considerations, or some combination of these factors makes it difficult to study such phenomena directly.

The self-awareness experiment was one of several simulations I have conducted with B. F. Skinner and others to try to investigate such recalcitrant behaviors; the project came to be called the "Columban [from the *Columba livia*, the taxonomic name for pigeon] Simulation Project" (Baxley, 1982; Epstein, 1981).

Rationale. The rationale, briefly stated, for this work is as follows: if you have reason to believe, based on principles of behavior established in the laboratory and information about a person's past, that certain experiences were responsible for the emergence of some mysterious behavior, you provide support for this conjecture if, after providing an animal that does not normally exhibit such behavior with these experiences, the animal exhibits similar behavior (Epstein, 1981). You can thus use animals to shed light on the possible contributions of certain environmental histories in the emergence of certain mysterious behaviors in humans. If your simulation is successful, you have not *proved* that the conjecture was correct—that the environmental history you identified is responsible for the emergence of the behavior in humans;

[2]Normal children and chimpanzees seem to be unique in that mere exposure to the contingencies of reinforcement that govern mirror use is sufficient to establish appropriate behavior (cf. Mans, Cicchetti, & Sroufe, 1978). Why the same does not occur with monkeys is a matter for further research.

rather, you have provided a plausible account of the behavior—what some philosophers call a "plausibility proof."

Adequacy. The adequacy of a simulation depends on a number of factors, and the set of pertinent factors varies with the domain of the simulation. The adequacy of the Columban simulations rests on five criteria, not all of which are met by all of the simulations.

First, if one makes use of certain techniques of conditioning or appeals to certain principles of behavior, the applicability of these techniques and principles to people must be shown. The greatest strength of the Columban simulations lies in the demonstrated generality of behavioral phenomena, such as chaining, discrimination, generalization, extinction, and so on, to scores of species, including *Homo sapiens*.

Second, the topography of the behavior in the simulation should resemble the topography of the simulated behavior; that is, the result should look right. In the self-awareness experiment, the pigeon's beak clearly moves toward a mark on its body that it cannot see directly; limbs aside, the behavior looks much like that of a chimp or child being subjected to the same test.

Third, the function of the behavior in the simulation should resemble the function of the simulated behavior; that is, the behaviors should occur for roughly the same reasons. Say we could get a pigeon to make a pecking movement toward the center of its breast simply by tugging on a tail feather. If we learned that during the mirror tests the tail feathers of our birds were being tugged, we would dismiss the results as uninformative. In fact, the birds pecked at their breasts because they had been taught to scan their bodies for blue dots and peck them and, as the various control conditions showed, because they spotted a blue dot in the corresponding position in the mirror. They did not peck simply because a mirror had been uncovered (uncovering the mirror while a bird wore a bib but no dot did not result in breast-directed behavior). And they did not peck simply because they felt the dot or saw it directly (dot-directed pecks did not occur in the absence of the mirror).

Fourth, the more structurally similar the organism is to a human, the more adequate the simulation. The more dissimilar the organism, the greater the likelihood that the result is due to an interaction between the conditioning you have provided and peculiarities of that organism. Ideally, of course, one would test humans themselves. Chimpanzees would probably be the next best candidates. Pigeons are hardly ideal, but one can do much worse (see below). Pigeons are used, not because of significant structural overlap with humans, but for other reasons, to be discussed in the final section of this chapter.

Fifth, and most important, it is critical that humans have had the experiences you have identified; the more evidence you have that this is so, the more adequate your simulation. The self-awareness simulation is strong here in one respect and weak in another. As noted above, there is considerable evidence that chimps and children have acquired both of the repertoires we identified before they are successful in the mirror test; chimps and children are unique in that they can learn to use mirrors through mere exposure to the contingencies of reinforcement which govern mirror use (Epstein, 1985a; Epstein & Koerner, 1986).

Examples of other Columban simulations follow:

SYMBOLIC COMMUNICATION

Savage-Rumbaugh, Rumbaugh, and Boysen (1978) reported what they claimed to be the first instance of "symbolic communication" between nonhumans—two chimpanzees. Though extensive training was necessary to establish the simple exchange, the authors attributed it, not to the training, but to the "knowledge," intentions, and flow of information between the chimps. An account in terms of conditioning would have been a clearer statement of what had been achieved. We made the point by setting up a similar exchange between two pigeons (Epstein *et al.*, 1980). After 5 weeks of training, one pigeon would, loosely speaking, "inform" another about a hidden color by pecking the corresponding black-on-white letter (Figure 2). We claimed in the published report that a similar history of conditioning could account for "comparable human language." Though the exchange does not measure up as a serious simulation, we have no reason to doubt the validity of the claim.

THE SPONTANEOUS USE OF MEMORANDA

In the symbolic-communication experiment one pigeon had functioned as a kind of speaker; it "said something about" a hidden color. The other was a kind of listener; it waited for and made use of a symbol provided by the speaker. We reversed the positions of the birds and trained each in the opposite role. Then we removed the restraining partition and, without any further training, placed each bird alone in the chamber so that it had access to both panels at once. Having learned to behave as a speaker and a listener in this situation, would it somehow talk to itself?

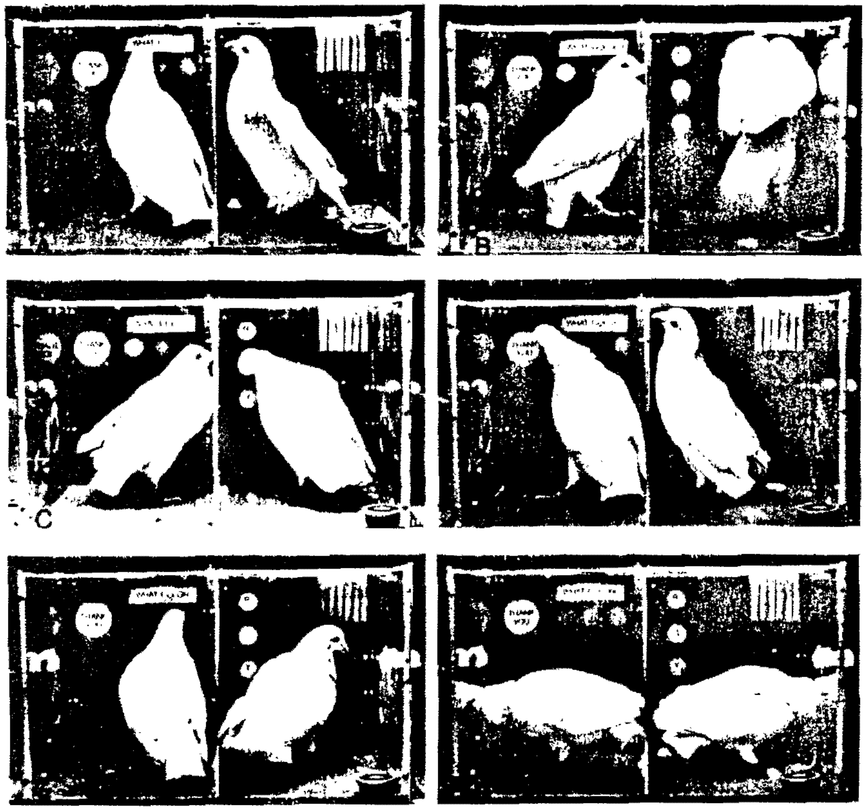


FIGURE 2. Typical communication sequence. (A) Jack pecks (and thus illuminates) the WHAT COLOR? key. (B) Jill thrusts her head through the curtain and pecks the color illuminated there (red, green, or yellow). (C) Jill pecks the corresponding letter (in this case, G for green), as Jack looks on. (D) Jack pecks THANK YOU, which operates Jill's feeder, as Jill looks on. (E) Jack pecks the corresponding color (in this case, green), which operates his feeder. (F) Both birds eat. The color keys below the WHAT COLOR? key are yellow, red, and green, respectively. The symbol keys are black-on-white.

After a few minutes, each bird came to display repeatedly the same stable sequence of responses. Elements of the speaker and listener repertoires came together to produce new, functionally distinct behavior that can reasonably be called memorandum-making. A bird would thrust its head behind the curtain on the right side of the panel and peck the hidden color, then peck and thus illuminate the corresponding black-on-white letter, behaving as a speaker. Then it would cross to the left-hand panel, often *look back* at the illuminated letter, and then peck the corresponding color (Figure 3). It appeared that the birds were using

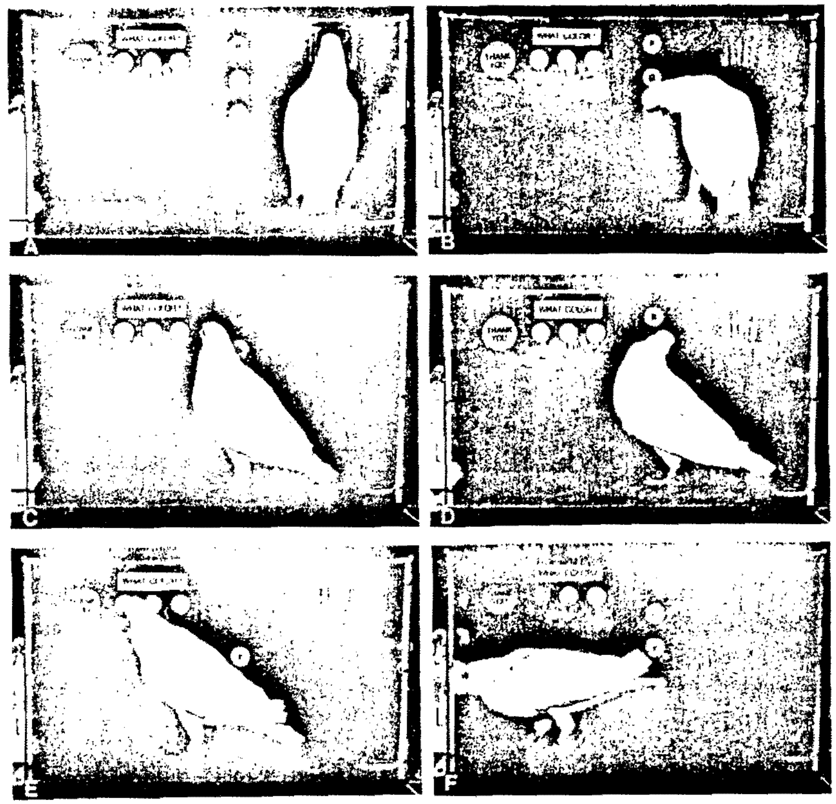


FIGURE 3. Use of a memorandum. (A) Jack pecks the color hidden behind the curtain. (B) Though doing so is not required, he pecks the corresponding letter (in this case, Y for yellow), which illuminates it. (C) He walks to the color keys. (D) He looks back at the illuminated letter. (E) He pecks the yellow key, which operates his feeder. (F) He eats.

the symbol keys as humans use memoranda, in this case to bridge the delay between the sight of the hidden color and the opportunity to peck the corresponding color key on the left-hand panel. We conducted a series of tests over a 5-month period that convinced us that the birds were indeed using the symbol keys as memoranda (Epstein & Skinner, 1981).

We had witnessed what has come to be called *the spontaneous interconnection of repertoires.* Previously established behaviors can come together in new situations to produce new sequences of behaviors, behaviors that have new functions, or behaviors that have new topographies. The spontaneous interconnection of repertoires is one of four probable sources of novel behavior in humans and the one, most likely, which accounts for novel behavior of the sort we usually consider the most mysterious (Epstein, 1985a).

"INSIGHT"

We have simulated a classic problem from Köhler's classic *The Mentality of Apes* (1925). Köhler placed a banana out of reach in one corner of a room and a small wooden crate about 2 1/2 m from the position on the floor beneath it. After a number of fruitless attempts by all six chimpanzees in the room to jump for the banana, one of them (Sultan) paced rapidly back and forth, then suddenly moved the box half a meter from the position of the banana "and springing upwards with all his force, tore down the banana" (Köhler, 1925, p. 41). The solution appeared in about 5 minutes. Köhler attributed the behavior to a mental process—the "insight" of the chimp.

We made some reasonable guesses about the origins of this behavior. Two repertoires seemed necessary: climbing on objects to reach other ones, and pushing things around. Because a pigeon normally does neither, it seemed an ideal candidate to test an environmental account of the chimp's "insight." We taught a pigeon (a) to push a small box toward targets at ground level and (b) to climb on a box fixed beneath a toy banana and then to peck the banana. We also placed it in the chamber with the banana alone and out of reach until brute force attempts to peck the banana (by flying and jumping) had extinguished. With the two repertoires established, we hung the banana out of reach in one corner of the chamber and placed the box in another corner—a new situation for the bird, not unlike the one that faced the chimps.

The bird performed in a manner that is remarkably chimp-like (and, perforce, human-like). It paced and looked perplexed, stretched toward the banana, glanced back and forth from box to banana and then energetically pushed the box toward it, looking up at it repeatedly as it did so, then stopped just short of it, climbed, and pecked (Figure 4). The solution appeared in about a minute for each of three birds (Epstein, 1981; Epstein, Kirshnit, Lanza, & Rubin, 1984). We have conducted controls that show that the climbing and pushing repertoires are necessary for the solution and have shown how different environmental histories contribute to success in the problem (Figure 5).

Based on these and other experiments, a tentative, moment-to-moment account of the performance can be given in terms of empirically

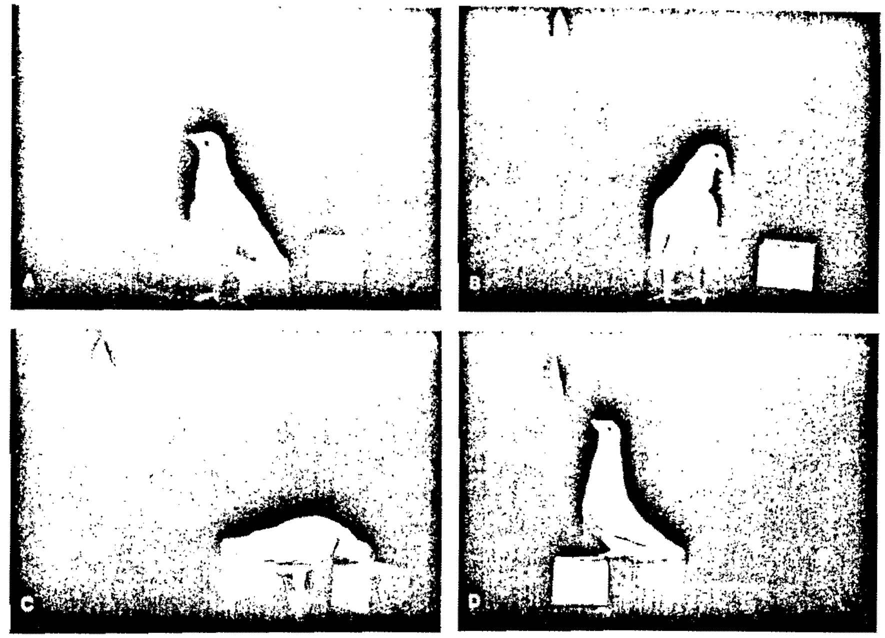
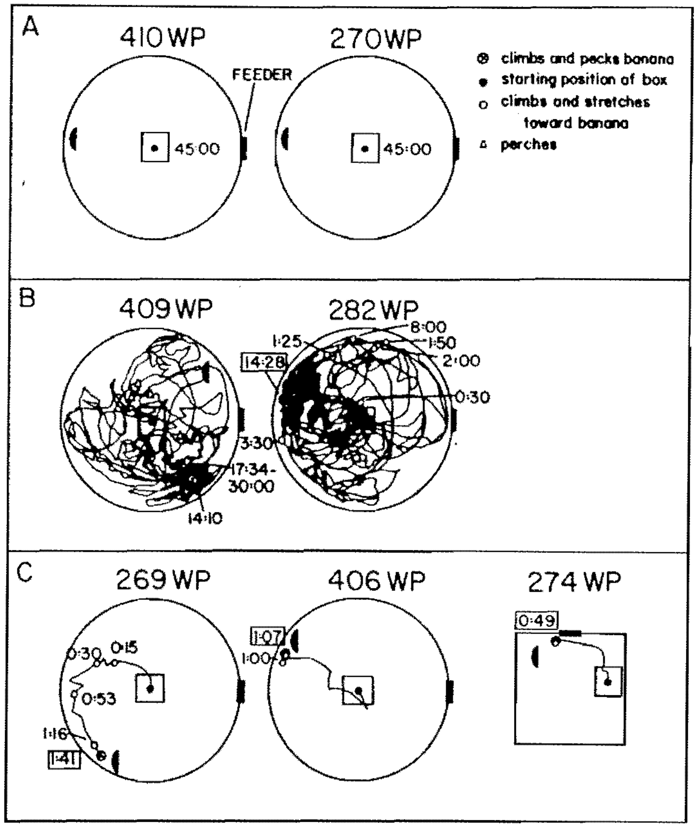


FIGURE 4. "Insight" in a pigeon. (A,B) The bird looks back and forth from banana to box. (C) It pushes the box toward the banana. (D) It climbs and pecks.

validated principles of behavior. At first stimuli are present which control both the climbing and pushing repertoires, and thus behaviors with respect to both the banana and the box appear, a phenomenon that may be labeled *stimulus matching*. The behavior we interpret as a sign of perplexity is probably the result of competition between the repertoires; the bird stretches toward the banana, looks over at the box, looks back at the banana, and so on. Behavior with respect to the banana quickly disappears primarily because of the recent history of extinction of "brute force" behavior; the pushing repertoire quickly gains in relative strength. Why the animal pushes *toward* the banana is a complicated matter. A process akin to what some call *functional generalization* (as opposed to generalization based solely on common physical characteristics) seems to be involved: Birds that have been trained to push toward a target but not to peck the banana do not push toward the banana in the test situation but do push toward the banana when subsequently trained to peck it.[3] In other words, the birds push toward the banana for the "right

[3]I am not, for two reasons, entirely happy with the term *functional generalization*. First, it implies an explanation, though at best it simply *describes* a spread of effect between stimuli which is not based on common physical characteristics. I *explain* the bird's behavior by



JRE 5. The contributions of various experiences to success in the "insight" experiment assessed by conducting the test with birds that had different training histories. For ıple, birds that had been trained to peck the toy banana but never to climb did not ɔ when the banana was placed out of reach above the box (not pictured). Birds that been trained to climb and peck but never to push did not push the box in the test ıtion (Panel A). Birds that had been trained (a) to climb and peck and (b) to push the aimlessly for long periods of time pushed the box over much of the floor space of the nber. The birds rarely looked up while pushing. One of the birds stopped pushing e appropriate place and climbed and pecked the banana after having pushed for more 14 min (Panel B). Birds that had been trained (a) to climb and peck and (b) to push ɔox toward a green spot placed at random positions along the base of the chamber ·ed the problem" efficiently and in a manner suggestive of human problem-solving ıvior (Panel C). For all of these animals, brute force attempts to reach the banana by ɔing and flying were extinguished before the test. Another bird was tested that had ı trained to climb and push toward the spot but whose "brute force" behavior had been extinguished. It jumped and flew toward the banana for several minutes but ıtually "solved the problem." Times are shown in minutes and seconds.

reasons"—because they have learned directional pushing and because some history of reinforcement has made the banana "important." The bird stops pushing in the right place because of a phenomenon called *automatic chaining*: In the course of pushing toward the banana, it sets up for itself at some point a stimulus (box-under-banana) that controls other behavior (climbing and pecking). It therefore stops pushing, climbs, and pecks (Epstein *et al.*, 1984; cf. Epstein, 1985b).

Other topics that have been investigated include learned and spontaneous imitation, cooperation, competition, reaction time as a measure of "mental processes," and "morality."

TOOL USE AND RESURGENCE

In one variation of the insight experiment, an element of what many would call "need" was introduced: The banana was placed within reach and pecking it was reinforced; the box was available in another part of the chamber, but the pigeon did not push it until it "needed to"—until the banana was raised (Au & Epstein, 1982).

In another experiment, a pigeon was confronted with a variation of the marble-under-the-couch problem: The pigeon, which had previously learned to push a box toward targets, appeared spontaneously to use a flat, hexagonal box as an extension of its beak—that is, as a tool—to touch a small metal plate that was out of reach behind a Plexiglas wall. (Pecking the plate had been reinforced when the plate was within reach.) Again, it did so only when it "needed to"—when the plate was no longer within reach. The details are noteworthy: The pigeon first stretched repeatedly toward the metal plate. After about 30 sec, it pecked weakly at the hexagonal box. It stretched again a few times toward the plate and then began somehow to look confused and even pensive. It pecked at the wall and floor. It looked back and forth from the box to the plate. Suddenly, after about 90 sec, it began to push the box directly toward the Plexiglas wall. When the box was under the wall, the pigeon lost control of it for a few seconds. It looked again at the plate, made

referring to its history (both pecking the banana and pushing toward the spot have been reinforced) and the current circumstances. Why such a history affects the bird in this way is a matter for the physiologist. The term has also been defined more narrowly than I have used it. Consider Bruner, Goodnow, and Austin (1961): "The problems of specifying the properties of objects that mediate a common categorizing response become less arduous when the category is a functional or utilitarian one. Rather than an internal state rendering a group of things equivalent, now equivalence is based on an external function. *The objects of a functional category fulfill a concrete and specific task requirement*—'things large enough and strong enough to plug this hole in the dike' " (p. 5, italics added).

some adjustments, and then pushed the box solidly against the plate and pecked it repeatedly (Epstein & Medalie, 1983).

A simple principle, called *resurgence*, can account for the behaviors that one might attribute to "need" in the experiments described above: *When, in a given situation, some response is extinguished, other responses that were reinforced under similar circumstances tend to recur* (Epstein, 1983, 1985c). Loosely put, when one response no longer pays off, an organism reverts to a response that used to pay off under similar circumstances. Thus, when the metal plate was moved out of reach, pecks to it were quickly extinguished. Older behavior—box pushing—got stronger as the first repertoire got weaker. As was the case in the insight experiment, the behavior from which we inferred confusion was probably produced by competition between the repertoires as they varied in strength (though the two repertoires were made available here through resurgence, not stimulus matching). Research is in progress which supports a general principle of resurgence, applicable not only to problem solving but to several anomalous findings in the literature on conditioning (Epstein, 1983, 1985c; cf. Enkema, Slavin, Spaeth, & Neuringer, 1972; Epstein & Skinner, 1980; Estes, 1955; Lindblom & Jenkins, 1981; Mowrer, 1940; O'Kelly, 1940; Sears, 1941).

The simulations have, as should be the case with any fertile program of research, raised more questions than they have answered: for example, is the interconnection of repertoires a random process? Would irrelevant repertoires have an equal chance of resurging in a problem-solving situation? The program has also provided a methodology for answering such questions.

COMPUTER SIMULATIONS OF COGNITIVE PROCESSES

Psychologists do not generally do the kind of simulation previously described. More common is the computer simulation—and not of behavior or of physiology, but of mental processes (e.g., Kosslyn & Schwartz, 1977; Newell & Simon, 1972; Simon, 1981). For example, Winograd's (1972) robot SHRDLU uses a sophisticated model of language processing to decipher the commands it is given. Anderson's (1972) FRAN is based on a model of human associative memory and can replicate some standard results of verbal learning experiments. Newell and Simon's (1972) General Problem Solver solves a limited class of logical problems (for example, in chess and mathematics) with human-like uncertainty. How do the Columban and computer simulations compare?

Adequacy. Computer simulations of cognition are inadequate in several respects. They live up best to the second criterion described earlier. The topography of the behavior of a computer is presumably its output; in a successful simulation the computer presumably produces output (protocols, diagrams, latencies, and so on) that resembles either some property of human behavior (e.g., latency) or some product of human behavior (e.g., a protocol). The function, however, of the behavior of a computer would seem to have little in common with that of human behavior. A computer's behavior is almost always rule-governed; that is, it is controlled by instructions. The behavior of organisms, on the other hand, is often multiply determined and, in particular, is often contingency-shaped (Skinner, 1966); that is, it is determined by the consequences of past behavior. A CRT that simulates a mental image (e.g., Kosslyn & Schwartz, 1977) does so because of a set of instructions that someone entered into the computer; whereas college sophomores respond in certain ways in a mental-imagery experiment because they have learned to speak English, because they have been given certain instructions and been asked certain questions, because they have been shown certain stimuli, and so on.

Computers and people would seem also to have little common structure. The anatomy and physiology of a pigeon are certainly closer to the anatomy and physiology of a person that are those of a computer. As Edelman (1982), a biologist, put it

> We are not clockwork machines, and we certainly are not possessed of brains that are like digital computers. We are part of that seamy web of natural selection which has itself evolved a selection machinery called our brain. (p. 48)

Because they are also products of evolution, presumably the same could be said of pigeons.

Finally, the history that one identifies in a Columban simulation—the origins of the behavior—is one that might indeed be possible for a human. No one would claim, however, that computer simulations of mental processes uncover anything about the origins of human behavior; it would be absurd to assert that a man behaves in certain ways because someone input a program into him.[4]

[4]A related argument is often made, but I think it is incorrect. Occasionally a program is equated with a kind of inner agent. Writes Edelman (1982), "In recent times, the brain has been looked at as a kind of computer. The difficulty with that view has to do not so much with the theory of computation as with the famous ghost that haunts all considerations of the brain, namely, the homunculus. Who, in fact, is telling whom what to do? Who is writing the program?" (p. 22). According to Skinner (1969), "There is a

Computer simulations of cognition, in short, may be plausible in the way they mimic human behavior but are adequate on no other grounds.

Other Problems. There are other reasons for objecting to computer simulations of cognition as tools for understanding human behavior or brain function (cf. Epstein, 1981). Even prominent cognitive psychologists have found reasons to object (e.g., Miller, 1981; Neisser, 1976).

Computer models of cognition are, virtually without exception, unconstrained by physiological data. They are not models of the brain (though such models have been developed—consider Edelman & Reeke, 1982). Some cognitivists defend this merely on the grounds that little is known about the nervous system; others go so far as to assert that physiological data are irrelevant to the study of cognition. You can, they say, discover the "software" that runs the brain—the "rules," the "instructions," the "organization"—without knowing anything about the hardware (consider Fodor, 1981; Simon, 1969). This assertion has several flaws.

First, it rests on a faulty characterization of software. Some cognitivists would have us believe that computer software does not actually exist in the computer—that it is the mental world of the machine.[5] But computer software has physical status—it is in no sense mental, metaphysical, or even particularly abstract. It usually exists as a magnetic array or a pattern of high and low voltages in a physical device. With the proper equipment and a translation table, one could literally read off one's software directly from the device. How a given pattern controls the operation of the machine and eventually produces certain output could in principle be established by running the machine very slowly—by "single stepping" it. In this sense, one might call the DNA of living cells "software"—highly compact, *physical* information that is critical in certain controlling operations. The "software" of the brain—a superfluous concept—can be found *in* the brain.[6]

Second, as any programmer can tell you, one can write a large number of different programs to do the same job (consider Moore, 1959). The issue has been brought to the attention of cognitive psychologists by Anderson (1978), who argues that pictorial and propositional accounts of mental imagery and indeed "wide classes of different representations" can be made to yield identical behavioral predictions and therefore that we can never decide between such models on the basis of behavioral data alone. The argument has been made in a different way in Quine's (1969) classic essay, "Ontological Relativity," in which he shows that an infinite number of mutually incompatible theories—not translatable one into the other—can be generated to account for the same data. Computer models of cognition will, in other words, most likely be dissimilar computer models.

Third, even granting that we could somehow deduce the existence of one and only one program by studying merely the behavior of our machine, the program would tell us nothing about the hardware—what it is made of, how to repair it, how to improve it, whether it uses Jacobson junctions or some other sort of gates; we would still have to start from scratch to learn where and how the program exists in the machine and how the machine works. In other words, Anderson's (1978) argument applies as well to hardware as it does to software. Even if it were possible to discover *the* program in cognition, it would tell us nothing about the brain.

homunculus in any machine built and instructed by men" (p. 61). But a program is a far cry from a little man inside the head; it is, as I discuss below, simply part of the structure of the computer which is critical to certain controlling operations—analogous, perhaps, to synaptic states in the brain. Cognitivists are not so naive as to think that there are homunculi in the head; the very attraction of the computer as a model of human intelligence is that the computer, once programmed, needs no helping hand to behave intelligently. The fact that the programmer is human is irrelevant to their position. An unprogrammed computer might be limited in its behavior, but so is the feral child; they were each produced and programmed by outside agents—mainly, people. An inner agent is no more necessary to the analysis of one than it is to the analysis of the other. The cognitivist is concerned only with whether or not the program is a good representation of the mental world, not with the origin of the representation.

[5]Simon (e.g., 1969) and others would have us believe that cognition stands in relation to the brain as molecular physics does to quantum mechanics—that is, that it is at a "higher level" of analysis. But unlike the "levels" at which we observe physical phenomena in biology, chemistry, physics, and their various subdivisions, cognition is rather difficult to locate. Just where and what is it? The word *level* is hardly a solution to the mind–body problem; nor should it justify scientific inquiry into the metaphysical. As I have noted elsewhere, the prayer of a cognitive scientist as he sits down before his computer terminal must go something like this: "Oh, Mind, if I have one, please reveal to me today the proper set of Rules—if there are any."

[6]Where software ends and hardware begins is not always clear. ROMs, for example, are storage devices from which one can only read. They are preset with instructions or data during manufacture. Is a ROM hardware or software? Hardware that contains software? More important, the instructions need not be represented in a magnetic array; they could literally be "hard wired": The modern equivalent of wires, relays, resistors, capacitors, and diodes, properly connected, could fulfill the same function that the program fulfills. One can have either a software or hardware "spooler," a hardware or software "latch," a hardware or software timer, and so on. In general, there is a hardware equivalent for every software function and vice versa.

Fourth, wanting to discover the program when you are working with a computer—though perhaps a thankless task—is not an unreasonable means for understanding its behavior, because a program is what you use to control a computer; it makes no sense to ask about its phylogenic or ontogenic histories. But we can control organisms only by manipulating the environment, genes, or the body; as I have indicated above, we will never be able to change line 455 in an instruction set in the mind. In that sense, computer models of mind can provide only the most trivial and ineffectual understanding of behavior, for they yield no means to control it.

Fifth, existing computer models encompass fairly narrow domains of human behavior, and there is little overlap between models. Models of attention, memory, imagery, language, perception, and so on, often have little in common, and Boden (1977, p. 444) has argued that more comprehensive simulations are in principle unattainable. Ironically, in the 17th century Descartes proposed a model of human functioning that was far more comprehensive than any existing computer model; he used his famous hydraulic metaphor to try to account for the emotions, thought, perception, sensation, and skeletal movements. His model was entirely hypothetical, of course, which made his task somewhat easier than that faced by today's computer modelers.

Sixth, as I have noted previously, rules may be entirely the wrong approach for representing human functioning. The behavior of a computer is truly rule governed. Its every action is governed by an instruction (LOAD, JUMP, POP, If A THEN B), and the instructions are stored in some form in the machine. Human behavior, too, can be governed by instructions: someone tells us where to turn ("Turn at the next corner"), or we read a recipe from a cookbook ("Add three eggs"), or we recite a rule that we have memorized as an aid to better performance ("Slow and steady wins the race"). But it is easy enough to envision intelligent systems that make no use whatsoever of rules, and no rules whatsoever need be stored in us—even the rules we recite aloud—for us to behave as we do.

Must an organism be equipped with a library of words, images, instructions, maps, and so on, to behave effectively in the world? Absolutely not. But clearly an organism is changed by its exposure to such things—changed in such a way that subsequent behavior will be different. An undergraduate exposed to a photograph in an imagery experiment on Monday will behave differently to similar photographs on Tuesday. How might we account for such a change without resorting to the representation and storage metaphors? What is the minimum picture we might paint?

Say that when some neuron (or group of neurons, or synapse, or group of synapses, or circuit, etc.) in a rat's (or undergraduate's) brain is in a certain state—call it the active state—the rat tends to flex its leg when exposed to the flash of a red light. And say further that this cell is normally inactive but that we can make it active simply by pairing the flash of a red light with the application of a shock to the rat's leg. *Voilà.* We can, by this operation, change the rat so that, in the future, when it is exposed to the flash of a red light, it will flex. Note that when the rat is so changed, it contains no rule about the new relationship that has been established between an environmental event and an event in its behavior. True, we could describe the relationship with a rule: "When you see a red flash, flex." But the cell is not such a rule; nor does it contain one. The active cell is in no sense analogous to the computer instruction; at best, it is analogous to a "flag" in a computer memory. But a flag is a far cry from an instruction. And the cell is not the red light, either, nor an encoding of it. It is simply the simplest possible manifestation of change in an organism which can effect subsequent behavior in meaningful ways.

As Epstein (1981) has noted, the stimulus that produces a change in us need not in any fashion produce a change that corresponds to the stimulus, for *to produce a change* is not necessarily *to produce a correspondence*. The change sometimes manifests itself, of course, in behavior that in some sense corresponds to the stimulus, but the nature of the change is simply not yet known.

Information Processors. The major problem lies with the assertion, which somehow always remains unanalyzed, that humans are "information processors"; that the human brain (or mind?) is an instruction-driven symbol system; that, in short, we work like computers. An *American Scientist* article is flagged, "*When considered as a physical symbol system*, the human brain can be fruitfully studied by computer simulation of its processes" [italics added]. Newell and Simon (1972) assert, "programmed computer and human problem solver are both species belonging to the genus IPS [Information Processing System]" (p. 870). It is true that programs can be written that get computers to behave in some (usually trivial) respects like people do. But one commits an error of logic in asserting from that fact and in the absence of other evidence that computer simulations of "cognitive processes" shed light either on the brain or on human behavior.

The major flaw in modern cognitive science can be reduced to a single syllogism, one that pervades the literature in this field. From Premises (1) and (2) below, an invalid inference is drawn:

Premise 1: All computers are entities that are capable of behaving intelligently.
Premise 2: All computers are information processors.
Conclusion: All entities that are capable of behaving intelligently are information processors.

In other words, all A (computers) are in the set B (entities that are capable of behaving intelligently); all A are in the set C (information processors); therefore, B is contained in C; or

$$[(A \supset B) \cap (A \supset C)] \supset (B \supset C)$$

Sometimes a more modest assertion is made: Because all D (human beings) are in B, all D must be in C (Figure 6); or *Homo sapiens* is a "species belonging to the genus IPS"; or

$$[(A \supset B) \cap (A \supset C) \cap (D \supset B)] \supset (D \supset C)$$

Note that although these expressions are false and the conclusions invalid, the conclusions may still be "true." Symbol manipulation may be the basis of all intelligent behavior ($B \supset C$) or at least all human behavior ($D \supset C$). But, as things stand, there is no evidence to support these conclusions; in other words, they are drawn (incorrectly) entirely from the premises. There is ample reason, on the other hand, to be skeptical about a characterization of people in terms of programs and symbols.

As long as the primary assertion of cognitive science remains unsupported by independent evidence, computer models of mind will tell us only the obvious—how we can get information-processing systems to behave like people.

PIGEONS

Why pigeons? As in most laboratory sciences, one starts one's investigations with the materials at hand. Pigeons have been used for many years in behavioral psychology because they are inexpensive, highly resistant to disease, and easy to handle; because they often live 15 or even 20 years in captivity; because their visual sensitivity is similar to that of humans; and because many of the behavioral processes that have been identified in pigeons have been shown to be applicable to humans

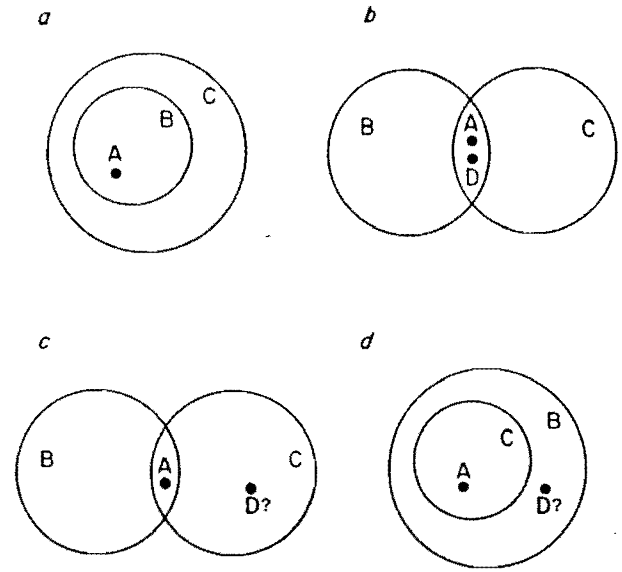


FIGURE 6. Venn diagrams that represent variants of the syllogism described in the text. A is the set of all computers. B is the set of all entities that are capable of behaving intelligently. C is the set of all information processors (for our purposes, the set of all entities whose behavior is governed by an instruction-driven symbol system). And D is the set of all human beings. An assertion that pervades the literature in cognitive science is that B is contained in C (Diagram a). A more modest assertion, implied by the first, is that D, the set of all people, necessarily lies in the intersection between B and C (Diagram b). Neither assertion is supported by evidence, however, and there is ample reason to be skeptical of both assertions. Though A is contained in both B and C, and though D is contained in B, the membership of D in C is uncertain (Diagram c). One could also argue that all Cs are contained in B (that all information processors are capable of behaving intelligently), but D might still lie outside of C (Diagram d).

and other animals. Pigeons, unexpectedly, proved to be good candidates for the Columban simulations precisely because they are so different from people. Because there is little physical resemblance and because the history and current conditions controlling a pigeon's behavior are apparent or at least accessible, one is less tempted to anthropomorphize than one might be with more human-like animals. The tendency to anthropomorphize in work with chimpanzees has been costly. It has, on the one hand, led to many instances of overinterpretation to which ethologists, linguists, and psychologists alike have objected (e.g., Chomsky & Premack, 1979; Epstein, 1982a; Epstein, Lanza, & Skinner, 1980;

Sebeok & Umiker-Sebeok, 1980; Terrace, Petitto, Sanders, & Bever, 1979), and it has obscured an account of the conditions that actually produce complex behavior in chimpanzees.

A point mentioned briefly earlier is worth emphasizing. It would be fatuous to assert that human behavior and pigeon behavior necessarily have the same causes. A history of conditioning that leads to the emergence of novel, interesting, human-like behavior in pigeons is not necessarily responsible for comparable human behavior; conditioning may not even be necessary for the human's achievement. The account becomes increasingly plausible, however, as one establishes the generality of behavioral principles, as one demonstrates that humans have indeed had certain experiences, and so on. Though pigeons are a good starting point for the investigation of certain complex human behaviors, one should hardly limit one's investigations to pigeons.[7]

CONCLUSIONS

Frederick II was a competent scientist, though irresponsible by current standards. We who are less bold can still shed light on the emergence of some otherwise mysterious human behaviors. Where a direct attack is impossible, we can construct plausible accounts of the emergence of certain complex human behaviors through careful simulations. Such simulations have so far revealed the possible role that certain complex histories of conditioning play in the emergence of novel behavior and have called attention to several behavioral processes that have received relatively little attention in laboratory praxics.

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[7]Hake (1982) and others have noted, as I did early in the chapter, that some domains of human behavior seem to be so uniquely human that animal studies can shed little light on them. Where, however, such behavior is derivable from simpler behaviors or general processes, animal studies can still be useful. Studies that employ animals to explore complex, typically human behavior are growing in both number and scope; animals studies have been proposed to study even subtle verbal processes (consider Catania, 1980). I do not think we yet fully appreciate what animals can tell us about complex behavioral phenomena.

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